

Author's response to the review

Tuomi, L., Johansson, M., Twelves, A., Rantanen, M., Lagemaa, P., Kanarik, H., Särkkä, J., Raudsepp, U., and Westerlund, A.: Record low sea levels in the Bothnian Bay in November 2024, EGU sphere [preprint], <https://doi.org/10.5194/sp-2025-12>

We would like to thank the two anonymous Referees for their constructive comments and suggestions to improve our manuscript. We have revised our manuscript accordingly. Please find below our detailed answers to the questions and comments. The referee comments are shown with italic font and our replies with plain blue text. All changes are made visible in the revised manuscript "Author's tracked changes".

RC1, Anonymous Referee #1, 28 Oct 2025

Summary: The manuscript presents a characterisation of an extreme low sea level in the Gulf of Bosnia in the Baltic Sea. The magnitude of this extreme was exceptional compared to the past historical record. This extreme event was linked to winds flowing from a specific direction. The study estimated the return period of this extreme and assessed the short-term forecast for it.

Recommendation: The manuscript is clearly written, and the analysis is, in my opinion, correctly conducted. I have, however, some concerns about the suitability of the topic for the journal, a decision that the editor or the editorial board should make. The manuscript presents an analysis of a very specific sea-level extreme. This assessment may certainly be relevant from the operational meteorological or oceanographic point of view, but I do not clearly see its impact as an assessment report for 'State of the Planet'

We thank the Reviewer for constructive comments and suggestions. As this paper is intended for OSR10, Chapter 4: Specific Events in the Ocean (2024), its main aim is to provide a detailed overview of the extreme sea level event that occurred in the Baltic Sea in November 2024 and to examine the factors that contributed to it. We have revised the manuscript to include information and time series of observed annual minima from the Kemi tide gauge (Figure 3). We also give more detailed description about the general forecast accuracy of the BAL MFC NRT system, allowing comparison between the accuracy of the extreme event forecast and the overall performance of the NRT system. We also discuss how these forecasts could have supported sea-level warnings issued by weather forecasting centres in this region. Please also see our detailed answers below.

Leaving this question aside, I have a few suggestions that the authors may want to consider:

1) The abstract indicates some sea-level figures to characterise the magnitude of the extreme, but these are only meaningful to researchers working specifically in this geographical area. Also, it would be more useful to indicate the magnitude in terms of standard deviations derived from some climatological period (possibly detrended to

subtract the impact of the GIA)

We have rewritten the abstract to better describe the content of the manuscript with focus on the extreme event in the northern part of the region.

2) The Abstract should also mention the exact date of the extreme. This is first mentioned at the end of the introduction.

We have added the exact data of the extreme event to the abstract.

3) line 96 'We present winds measured at Kalajoki...' daily, hourly, 6-hourly..?

The wind measurements presented are hourly 10-min averaged values. We have added this information to the manuscript in Section 2.

4) The resolution of Figure 1 should be improved.

We apologise for the poor resolution of Figure 1. Something went wrong in the conversion of this Figure from the doc-version to the pdf-version. In the revised version of the manuscript we have taken care that the resolution does not change in the conversion.

5) line 170 'forecast lengths' I think the authors mean forecast horizons. For instance, the length of a forecast be 10 days if it's continued beyond the forecast horizon of, say 1 24 hours.

We thank the reviewer for this comment and have revised all instances in the text to ensure consistency: when referring to the total length of the forecast, we use the term forecast length, and when discussing the time between the start of the forecast and the time of the event, we use forecast lead-time. We hope this change makes the text clearer and easier to read.

6) The GEV analysis is correctly conducted using detrended data. It is interesting to know whether November 21st remains a record sea level even in the detrended series

We have added time-series of observed annual minima from the Kemi tide gauge including also the detrended dataset in Figure 3. The 21 Nov 2024 event is a record also in the detrended dataset.

7) A GEV analysis of the winds, for instance, daily means, would also be interesting. It would address the question of whether the sea-level record was due solely to the magnitude of the winds, or to some combination of strong winds, the prefilling state, or sea-ice cover.

Thank you for this suggestion. Due to the small size and complex shape of the Baltic Sea, wind direction plays a key role in extreme events. Thus, the record-low sea level in

Kemi was not due to the magnitude of wind speeds alone, but to the combination of strong wind speed and northeasterly direction that pushed water away from the Bothnian Bay. A GEV analysis of wind speeds would therefore be insufficient; such an analysis would need to be restricted to winds from the northeast sector. Isolating this effect carefully would require substantial additional effort and is beyond the scope of the present study, although we agree it would be an interesting topic for future research.

8) What were the sea-ice cover conditions? Ice cover is known to modulate the amount of momentum transferred from the atmosphere to the ocean. Did a smaller-than-normal ice cover play a role? Somewhat surprisingly, ice cover is not mentioned at all in the manuscript (or I missed it). This would increase the 'climatological aspect' of the study and perhaps make it more suitable for publication in State of the Planet

We thank the reviewer for raising the issue of momentum transfer modulation by sea ice, which indeed can have a substantial effect on sea levels in the Baltic Sea. At the time of the event (21st November 2024) there was only thin sea ice at few coastal regions of the Bothnian Bay. Thus, the dampening effect of sea ice did not have a significant impact on the event. We have added information about the sea ice situation to the manuscript.

RC2, Anonymous Referee #2, 12 Nov 2025

This paper explores the accuracy of the Finnish Baltic Sea Monitoring and Forecasting Centre native model (named NRT physical system) in reproducing observed sea levels during a severe windstorm in November 2024. Model sea surface heights are compared against nearby tide gauge observations. The manuscript assesses the NRT physical system model accuracy under different forecasting lead times (12-24 hrs / 36-48 hrs/ 60-72 hrs & 84-96 hrs), with the shortest lead times providing higher forecast accuracy. The authors found that the model tends to underestimate highs, and lows, thus not fully reproducing the extremes, particularly in the longer range forecasts. This is a known limitation of this version of the model, from prior literature. With the negative storm surge being recorded in high resolution by the tide gauges, the return period of this record-breaking sea level low is estimated, using measurements from Kemi tide gauge (in the context of the longer 1977-2024 record). Land uplift and sea level changes over the period are known and were used to detrend the data. Return period estimates are demonstrably more accurate if observed extremes, like this storm, are included in the analysis.

We thank the Referee 2 for these constructive comments and suggestions to improve our manuscript. We have added time-series of the Kemi tide gauge data including also the detrended data in Figure 3. More detailed information about the general forecast accuracy of the BAL MFC NRT system has also been added to the text. Together with

information about the FMI's warnings issued for low sea level events and how the accuracy of the forecasts fulfilled the criteria for those. We have also reformulated Table 1 to give better overview of the products and datasets used in this paper. Please also see our detailed answers below.

General comments and scientific issues:

This manuscript describing the modelling of a uniquely severe storm at the northern extremity of the Baltic Sea, in the Bothnian Bay and Gulf of Finland in November 2024, reads as a technical report of the event - the purpose of which is validating the Baltic Monitoring and Forecasting Center's Near Real Time model for extreme events. The topic of representing extreme sea levels due to storms should be of great interest for modellers, and is a topic of current interest in our changing climate. The goals of the paper are neatly set out and indeed addressed. The methodology and results are clear and well presented, although Table 1 may require some improvements to the headers, and description, for clarity (e.g. CMEMS is not defined in the report. The Product ID & Type descriptions are a little confusing).

We have made some clarifications for Table 1 to better describe the datasets used in this paper. We have also checked that all the acronyms have been explained either in the Table or in the text.

Also the role of the Automatic Weather Stations (lines 95-103) in the procedure may need clarifying. Data appears to be accessible for experiments to be reproduced.

The Automatic Weather Station (AWS) data was used in this study to give an overview of the measured wind conditions during the storm event. The 1-hourly data we have used in this study is available from FMI's open data server. We have added Data availability section to the manuscript, which describes in more detail how to reach this data and also other datasets used in the manuscript.

Results do certainly support the interpretations and conclusions. The entire manuscript is well structured and findings are concisely presented. As such it is a nice read. However, the authors may wish to consider posing a scientific question or hypothesis to elevate the motivation of this manuscript for the readership. The innovation, or novelty, or new conclusions, presented by this paper was not clearly stated. I imagine few extreme negative storm surges are captured for model calibration and development, therefore a discussion of why the model had difficulty, and the things that could be considered to improve the model to better represent and forecast such events (even to longer lead times) would be an interesting contribution to the scientific knowledge. As such, it may benefit the manuscript if the discussion and conclusions sections are reworked to provide new insights, or discuss the unique problems of modelling such extreme events, alongside the unique tide gauge observation data and unique storm event.

We have added further information and discussion on the predictive capabilities of the NRT system in producing SSH. We have also clarified how forecast accuracy varies with lead time, distinguishing between contributions from the BAL MFC model setup and atmospheric forcing. To provide context for the reported accuracy metrics, we added discussion about the NRT system's ability to deliver sufficient information for issuing weather warnings.

There are a number of abbreviations and initialisms presented in the report, and as such, it may also be beneficial to consider an end of report Glossary for reference.

We have checked that revised version of the manuscript includes explanation for all the abbreviations used in the text, figures and tables